

*Brunns (J. D.)*

# ON THE FEVER

OF THE

# Lower Coast of the Mississippi River,

DURING THE SUMMER OF 1880.

*Being an Examination of the Reports on the same:*

READ BEFORE THE

AMERICAN PUBLIC HEALTH ASSOCIATION,

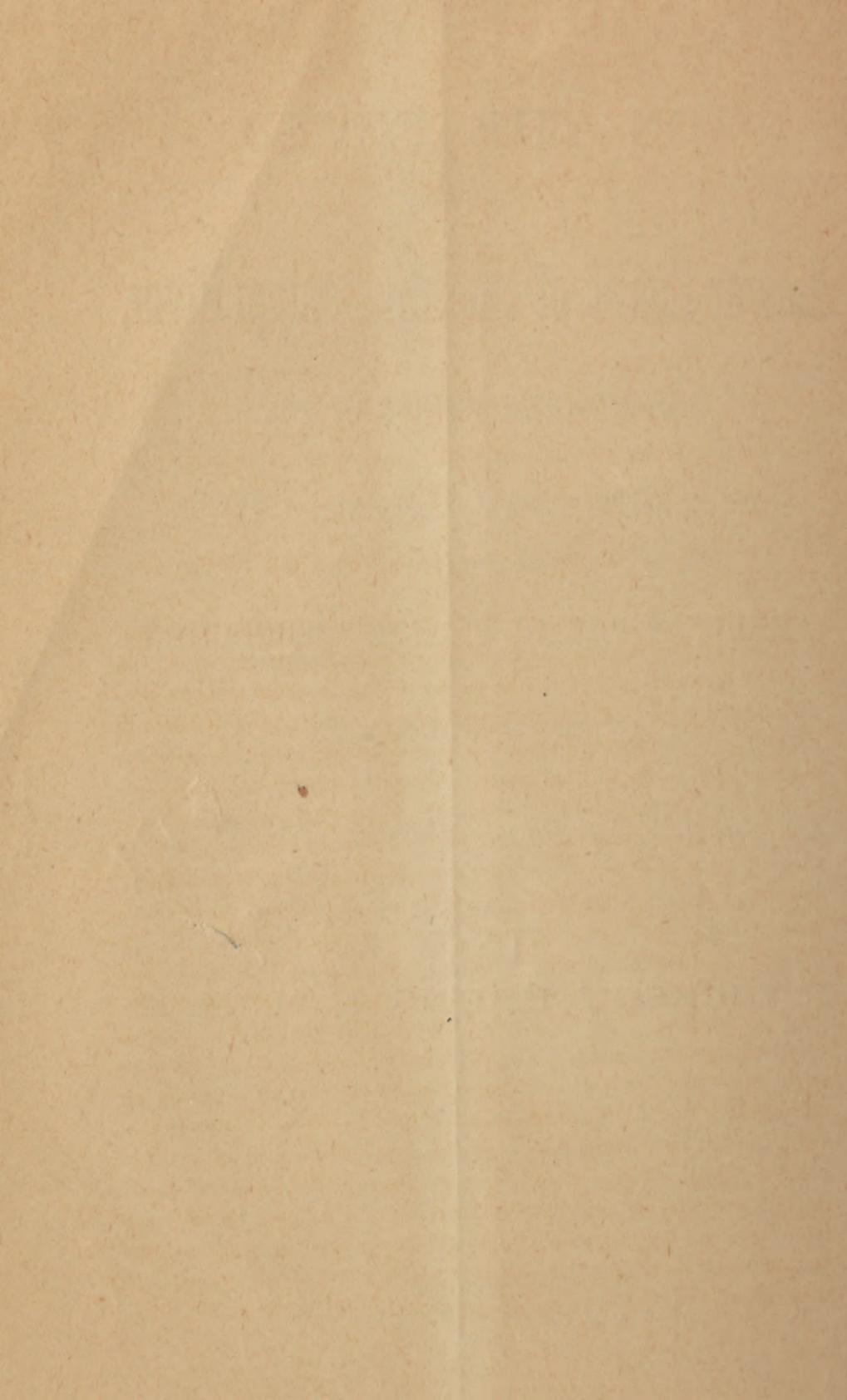
AT THEIR ANNUAL MEETING

Held in New Orleans, December 7, 1880,

— BY —

J. DICKSON BRUNS, A. M., M. D.





# PAPER

READ BY

J. DICKSON BRUNS, A. M., M. D.,

*Before the American Public Health Association, on the Fever of the Lower  
Coast of the Mississippi River,*

AT ITS ANNUAL MEETING, DECEMBER 7, 1880.

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*Mr. President and Gentlemen of the American Public Health Association:*

Supplement No. 8, of the "National Board of Health Bulletin," is largely made up of the reports of special commissions and of officers of the National Board on the character of the fever which has prevailed during the past summer on the lower coast of the Mississippi River. As a member of one of those commissions I desire to say some words—final, so far as I am concerned—on a question of serious import to the future of New Orleans. In doing so, I ask the indulgence of the convention in advance for the apparent immodesty of any allusions to myself which the exigencies of the inquiry may demand, and I disclaim, emphatically, any intentional courtesy in such allusions to others as the gravity of the subject may require.

I have no taste for medical polemics. It is a field in which our profession, owing to some inherent defect of its qualities, has gathered few laurels. Nor, deeply as I deplore the want of harmonious co-operation between our State and national boards, do I intend even so much as to allude to the cause or results of their unfortunate antagonisms. But the National Board of Health has been pleased to adopt, with singular unanimity, what I may, for the sake of brevity, call the Sternberg report, and the same sense of public duty which made me accept, at considerable sacrifice, the request to act as a member of an investigating committee, forces me to discharge the further duty towards this community of examining the grounds of that report and of the official adoption of it. This great city, Nine-

veh, is not to be destroyed because there is a worm at the root of some prophet's gourd.

Most of the convention, I suppose, are aware that in the early part of last August a fever made its appearance in the parish of Plaquemines, which, from its exceptional mortality in one family, (the Giordiano, four out of five children attacked with it dying), excited some alarm in the city, and led the resident member of the National Board to request Dr. G. M. Sternberg, of the United States Army, to examine and report upon it. He did so, and declared his belief that it was yellow fever. His conclusions not being accepted by the State Board of Health, which declined to accept Dr. Bemiss's offers of aid for purposes of disinfection, etc., that officer, with the approval of the Auxiliary Sanitary Association of New Orleans, appointed another commission, consisting of Dr. J. Dickson Bruns and Dr. J. P. Davidson, of New Orleans, and Dr. G. M. Sternberg, U. S. A., to make a more critical examination and report.

On the fifteenth and sixteenth of September that committee visited the infected district and made two reports; a majority one signed by Doctors Bruns and Davidson, declaring the disease to be the endemic malarial fever of the country; and a minority one signed by Dr. Sternberg, reaffirming the opinion expressed in his previous report.

Dr. Bemiss, on the receipt of the two reports, declared that he would make the majority report the basis of his report to Washington. The public was therefore naturally surprised when the official report to the secretary of the board appeared, signed by S. M. Bemiss, member National Board of Health, New Orleans, La., and R. W. Mitchell, member National Board of Health, Memphis, Tenn., adopting Dr. Sternberg's views, and concluding with the following sweeping affirmations:

"They (Doctors Bemiss and Mitchell) cannot, however, close this report without placing themselves on record as fully accepting and indorsing Surgeon Sternberg's conclusions, to-wit :

"1. That yellow fever (about 100 cases) existed between August 1 and September 10 in Plaquemines parish, La.

"2. That the outbreak had its origin in the immediate vicinity of the Mississippi River Quarantine Station, the first case, August 1, occurring directly opposite the point where the infected Bark Excelsior was detained from July 11 to August 16. That while the type of

the disease was generally mild, vicious local conditions existed which aggravated it into the most fatal form, four dying in one family out of five attacked."

The resident member for New Orleans explained, however, on interrogation, that the report, which seemed to be his, was written by Dr. Mitchell to whom he had delegated the sole discharge of this duty, and that when sent to him for signature he had, through some inadvertence, overlooked the word *yellow* in the first affirmation. Furthermore, that he had immediately telegraphed to Washington to stop the publication of the report, but received answer that it was too late, as it was already in press.

We may, therefore, without injustice to any one, designate this report hereafter as Dr. Mitchell's report. Now, from its general tenor, from one or two expressions in it, and from the absence of any declaration to the contrary, the reader would infer that Dr. Mitchell had arrived, through *personal* observation, at an independent conclusion concerning the nature of the fever in question.

Such was evidently the impression made upon the secular press everywhere, and not improbably upon the president and other members of the National Board. And as such additional testimony could not fail to carry with it great weight, I think it important to note that Dr. Mitchell's conclusions were not the result of any personal investigation whatsoever, and are not entitled, therefore, to the proper force which this would have given them. On the contrary, his abstention was so marked that Dr. Davidson and myself soon noted it, and on being invited to join us in our domiciliary visits, Dr. Mitchell said to Dr. Davidson that he "had only accompanied the commission as a mark of courtesy to it, and being a member of the National Board he thought that under the circumstances it would be best that he should take no part in the investigation"—which he studiously refrained from doing.

The field of our survey, therefore, is narrowed to the majority and minority reports alone, with such light as may be thrown upon them by the testimony of the local practitioners. And the conclusion of the National Board, grave as the results of its official declaration may be, is clearly entitled only to such respect as it derives from the force of the evidence upon which it is based. That this is not overwhelming one fact, I think, indicates, at the very threshold of the inquiry, namely the unnecessary and exaggerated claims, which are setup for the

author of the minority report as a very high authority on yellow fever. If the question was one to be settled by *authority*, the second commission was wholly useless ; if by evidence, the only qualification needed were truthfulness, capacity and freedom from bias.

But Dr. Mitchell prefaces his report by saying that "Surgeon Sternberg was selected for this duty"—among other reasons—"because of his *extensive and intimate acquaintance* with the fevers of the Gulf Coast and Spanish main." And the president of the National Board of Health, in *his* report tells us that "Dr. G. M. Sternberg, of the United States Army, has had a *large experience* in yellow fever epidemics and has made a special study of the character of the disease both in the United States and in Havana." The other members of the commission, for any official recognition of their competency or standing in this court, might have been last spring's graduates.

To lay such stress upon the *a priori* value of one expert's opinion, and to utterly ignore the reasonable claims of his co-workers to the recognition which their very appointment would seem to presuppose, looks as if it were feared that the evidence alone would not bear the strain intended to be put upon it.

Mr. President, I am jealous of no man's reputation. I envy none any compliment, however high, that may be deservedly paid him. If he has won his title to pre-eminence honestly I rejoice to see him wear it loftily. To all that can be said of Dr. Sternberg as a distinguished member of a most honorable service, I yield a cordial and cheerful assent. But as far as I have been able, without indelicacy or intrusion, to ascertain Dr. Sternberg's personal acquaintance with "the fevers of the Gulf coast and of the Spanish main," is limited to the observation of a few cases at the Pensacola navy-yard in 1874; an outbreak at Barancas in 1875, when his study of the disease was cut short by an attack of it, which forced him to leave the post to complete his convalescence, and to such opportunities as he may have enjoyed as a member of the National Commission in Havana during the exceptionally healthy summer of 1879. May I, without incurring the charge of flagrant immodesty say—what the candor of others should have spared me—that Dr. Davidson, one of our very oldest practitioners of medicine, a gentleman distinguished for the rare freedom and calmness of his judgment, and for his delicate, alert and penetrative powers of observation, has had an experience,

in the diagnosis and treatment of yellow fever in Louisiana, which goes back for more than half a century. An experience, that is to say second to that of no medical man in the United States. For myself it will suffice to add that since my study of yellow fever began I have seen eight great epidemics of it, in South Carolina, Louisiana and Georgia. And both of us, in the sporadic yellow fever which prevailed in New Orleans for nine successive summers—from 1867—have been trained in a far more delicate and critical school of differential diagnosis than any number of declared epidemics could possibly afford. Nay more, in such a differential diagnosis as is demanded by the very terms of the proposition before us, a knowledge of malarial fever in its many subtle and obscure forms is as essential as a knowledge of yellow fever. Dr. Sternberg's opportunities for the study of these must have been necessarily limited by the demands of his service, whilst both the other members of the commission have spent every hour of their active professional lives in malarial districts.

If, then, Dr. Sternberg's experience has not been such as to justly give a preponderating authority to his mere opinions, let us examine the evidence submitted as a basis for these opinions.

Of his first report, September 10, I have only to say that after a critical study of it I do not find, in the evidence submitted, a single fact to warrant the conclusion that the fever was yellow fever. Its emphatic decision certainly justified the Resident Member for Louisiana in taking further steps to satisfy himself and the public as to its nature, and to prevent, if necessary, its spread; but, aside from some general statements whose accuracy and value I shall presently discuss, the finding of albumen in the urine of three, cases a "suspicion of coffee-ground vomit" in another and fatal case, and seeing a cadaver—the body of a mulatto whose "natural color was," in his own words, "light yellow," and in whom suppression of urine was said to have occurred sixteen hours before death, it not being stated whether this was verified, as is extremely improbable, with the catheter—there is not, I repeat, any evidence whatsoever in the entire report from which an unprejudiced expert, sitting in judgment on so grave a question, could honestly give a verdict. Dr. Sternberg, of course, does not and could not claim that it must be yellow fever because four out of six children attacked in one family had died, for

in the close of his report he has very truly though unnecessarily said that it is "extremely unscientific to make our diagnosis depend upon a greater or less mortality."

Dr. Sternberg's second report is founded upon the observations made by us *conjointly*. Let us first examine the history of these cases before analyzing the generalizations he bases upon them. As their *anamnesis* is given only in the majority report, and as its accuracy has not been questioned, I shall follow the descriptions given in that. There are but seven of which note was specially made, and of which discussion, therefore, is possible.

The first is "Paul Gravolet, aged twenty-two years, who was taken on the afternoon of the tenth with a chill, followed by a violent headache, pain in the loins and legs, nausea, retching and fever. Dr. Herbert visited him for the first time on the twelfth instant at 8 a. m., and found him suffering from fever, with a hot dry skin; temperature not noted. The vomiting of bile and mucus continued; the eyes were congested; the tongue moist, streaked in centre, red at tip and edges, was covered with a white fur; the fur had disappeared on the fourth day, leaving the whole organ red; the gums were red and swollen, but firm; there was much restlessness throughout the attack; the respiration was tranquil, without sighing; he complained of slight pain on pressure over epigastrium; the urine had been abundant and free from albumen; there had been no delirium. The doctor could not say that he had noticed any decided remission at the period of his visits, morning and evening, but had sometimes found him perspiring. He had broken his thermometer and could not tell what the diurnal variations of temperature may have been.

At the time of our visit, 1:30 p. m., September 15, the pulse was 80 to the minute, temperature 101 1-5 deg. F., respiration normal, tongue clean, gums pink and firm, skin pleasant, warm and soft, presenting no harshness nor pungency to the touch; the face was flushed, without capillary congestion; the body was of the natural color, and neither it nor the eyes showed the least tinge of yellowness; the *facies* was perfectly calm and the patient cheerful. He complained of some pain on pressure over stomach and abdomen. At 11 a. m. he had passed a small quantity of bright, florid blood by stool. The urine was abundant. A fresh specimen, tested on the spot, yielded, on the addition of nitric acid, a light precipitate, which

cleared up perfectly on boiling. At our second visit, a little after noon the following day, we found him still convalescing. He had passed a little bright blood by stool during the night, but had slept well. The pulse was 60 to the minute, temperature 99 3-5 deg. F. No albumen in urine."

Can a more characteristic case of the milder form of bilious remittent fever be presented? With the first onset of fever, following the chill, nausea and retching and vomiting of bile and mucus begin. In yellow fever the stomach is almost uniformly calm at this period, it is not usually until the second day, or even later, that gastric distress manifests itself. In the case before us the pain in the epigastrium is slight throughout; the tongue is moist, with a soft, white fur; the urine is copious and free from albumen to the close of the attack. The doctor, having no thermometer, and visiting him only morning and evening, cannot pronounce authoritatively as to distinct remissions, but remembers that he had at times found him perspiring. On the fourth day he has a slight hemorrhage from the bowels; this is repeated on the fifth, and at the time of our visit, just at the close of the fifth day, we find him still with a temperature  $2\frac{1}{2}$  deg. above normal, with clean gums and tongue, a soft, natural pulse, a warm, pleasant skin, with no tinge of yellowness, and a cheerful facies.

The second case was that of "Pierre Dragou, a male white child, aged five years, the younger brother of Adrian, whose cadaver Dr. Sternberg saw. Three days before our visit he had recovered from an attack of fever; but two days after convalescence had partaken freely of sardines and chicken for breakfast, and at noon was seized with violent vomiting and purging. There had been no hemorrhage from bowels. The child was calm and cheerful; the skin soft and moist; the temperature, carefully taken in axilla, was 100 deg. F. The pulse of the little patient, much excited by our presence, was, by first count, 110 to the minute. At the close of our somewhat protracted visit, it had fallen to 92. There had been six cases of fever in this family. All had recovered but Adrian. A small specimen of the patient's urine, very dirty, full of hairs and mucus, was secured. It threw down a flaky deposit on the addition of nitric acid, not cleared by boiling. As a substitute for filtering paper a single thickness of newspaper was tried. I thought it a clear case of failure; but, if trustworthy, the urine contained a trace of albumen. At our visit in

the afternoon of the following day the patient was convalescent, though still somewhat feverish."

Now here is a child, convalescent two days from an attack of yellow fever, who brings on an indigestion from eating a breakfast of chicken and sardines—an *entire box* of the latter, we were told. He had a violent attack of vomiting and purging, and we find him the next day sitting up in bed, as cheerful as possible, with a pulse and temperature almost normal. He was *taken out of bed, and stood up* to pass his water; the small specimen secured was from a foul vessel and no reliance could be placed on any results it yielded. But when we consider the lesions of yellow fever, of the stomach and liver and of the gastro-intestinal radicals of the portal circulation, I may ask whether a patient in a relapse from over-feeding is likely to have presented the aspect of this child? I may go further and ask any one familiar with yellow fever if relapses from this cause do not constitute the most formidable, and almost always fatal, complication met with in this disease?

The next patient visited by us in this neighborhood was "Eliza Martin, white female, aged fourteen years. She had come from New Orleans on the afternoon of the tenth, and was taken with fever three days after. She had been treated with calomel and quinine. On the second day of her fever (Tuesday) Dr. Hebert reports a *well-marked remission* in the morning. When seen by us on Wednesday, at 3 p.m., at the close of the third day, her pulse was 120 to the minute, temperature 103 2-5 deg. F., the tongue soft, moist, marked by the teeth, covered with light white fur; the gums pale, pink, firm; *facies* calm; skin pleasant to touch and bedewed with slight perspiration. There was a tendency to diarrhoea and slight pain was complained of on pressure over abdomen. Urine, tested on the spot with nitric acid and heat, was free from albumen."

Is it possible, in this case with any show of fairness, to dispute Dr. Hebert's diagnosis of remittent fever?

The above three cases, I may add, recovered promptly, without relapses, and with no sequæ.

On the opposite side of the river, at the Franklin rice mill, we also visited with Dr. Hebert, a patient of Dr. Hays', "Michel Halceran, a native of Louisiana, white, male, married, aged 33 years. He had been taken on the twelfth, at 10 a. m., with chill, violent headache

and pain in back and legs, accompanied by gastric distress and vomiting. The last continued throughout his attack ; but we learned from his friends, and from his physicians later, that the gastric irritability characterized him even in health. He is a confirmed dyspeptic, vomits his food frequently and is unable to retain a dose of medicine unless it be disguised and concealed even from his suspicion. Dr. Hays informed us the next day that when he first saw Halceran, at 12 m. on Sunday—two hours after his seizure—the temperature was 103 deg. F. Of his temperature Monday he had no record and did not recall it, but on Tuesday he found it to be  $101\frac{3}{4}$  deg. F. at his morning's visit, and  $102\frac{1}{2}$  deg. F. in the afternoon. He said he had had yellow fever during the epidemic of 1867.

"At the time of our visit we found him perfectly free of fever. The temperature was 100 deg. F., pulse 62 to the minute, full, soft, slow ; tongue clean and moist ; no yellowness of conjunctivæ or skin. Auscultation of heart revealed a soft, aortic, systolic murmur. Further inquiry, afterwards confirmed by Dr. Hays, revealed the fact that the patient had suffered from more than one attack of acute rheumatic fever. The urine showed a small quantity of albumen, *probably persistent.*"

Was this yellow fever ? It cannot certainly be called a *continued* fever, for the temperature which on the morning of the third day had fallen to  $101\frac{3}{4}$  deg. F., rose in the afternoon of the same day to  $102\frac{1}{2}$  deg. F. There is no record of the temperature during the long interval of two days.

I suspected at the time that he was perhaps the subject of chronic cardiac or renal disease, as the hint that the albumen present in the urine was "probably persistent" shows. On the sixth of October Dr. Hayshad the courtesy to write me a letter, to which I shall have occasion to refer again, giving me the results in the cases seen by us on the fifteenth of September. Of Michel Halceran he says : "He became free of fever (as shown by the thermometer) on the sixteenth, and was nicely convalescing until the eighteenth. On that day, at 11 a.m., I found him still free from fever, but he stated that at 8 or 9 a.m., he had felt a flush of heat about his chest and was uneasy for a time lest fever should supervene. That sensation had passed off however. He chatted pleasantly and laughed gaily in my presence. I left his bedside to get a paper from the steamer just landed, and was not absent ten minutes, when I was hurriedly recalled to the house

by the announcement that 'Mr. Michel was dying.' I found him in an epileptic convulsion that lasted fifteen minutes, and in an hour that was succeeded by another. That night he had three more severe epileptic seizures, and they continued to recur, with greater or less intensity, until his death, on the twenty-first. On the twentieth he had two of an hour's duration each. After each seizure he became perceptibly weaker, and steadily refused to take medicine or nourishment during the last four days of his life. He did not vomit anything black during his entire illness, and did not vomit at all during the last five days. He was subject to epileptic attacks, one having occurred about two months previous to his being taken sick, and others at intervals before that. From the sixteenth to the twenty-first he had no fever. He died quietly and without a convulsion."

Now, whether the convulsions were epileptic or not, I dare affirm that the entire history of yellow fever shows no death of this character on the sixth day of a complete and continued apyrexia.

To proceed : "Thomas Gilmore, white, male, aged nine years, was taken at midnight Sunday with the usual light chill and pains in back and legs. Dr. Hays at the date of his first visit, about noon on Monday, found him with high fever, hot, dry skin, frequent, quick pulse, white, furred tongue, and free from nausea, or pain at epigastrium ; the respiration was slightly hurried, without sighing, and there was no jactitation. His temperature was 105 deg. F. The following forenoon it was 105 deg. F.; in the afternoon it was 104½ deg. F. At the time of our visit, 5:20 p. m., it was 103 deg. F. Although an unusually nervous child, his expression was placid, exhibiting neither alarm nor depression. The conjunctivæ were pinkish ; but there was no intolerance of light, nor pain on pressure over the eyeballs. The face was slightly flushed without capillary congestion. The color of the body was natural, and there was no yellowness of skin or eyes. To the touch the surface was dry and warm, without harshness or pungency. The pulse was 100 to the minute; respiration normal, no suspiria. The bowels had been freely moved, the dejections were natural, the urine copious. There was slight uneasiness manifested on pressure over the epigastrium and abdomen ; but he made no complaint, except of slight headache.

The following day we visited him again at 10 a. m. He had slept well ; had two rather thin stools during the night, the last at 4 a. m. ;

with both had passed urine freely. The pulse was 92, temperature 101 3-5 deg. F. *Facies* cheerful, skin pleasant. From the excessive nervousness of the little patient we could not secure a specimen of urine for examination at either visit."

"Thos. Gilmore's fever," writes Dr. Hays to me, on the sixth of October, "left him the day after you saw him. He had a distinct intermission of about five hours, then fever returned, rose rapidly and he died with all the symptoms of cerebro-spinal meningitis. He was the boy whose urine Dr. Sternberg failed to get, and in whom he suspected suppression. The evening before he died" (the date is not stated) "I catheterized him and drew off twenty-eight and one-half ounces of a clear, straw-colored urine; that on the addition of nitric acid and to heat, yielded no albumen."

Was this yellow fever, or that fatal type of malarial fever which, in the very hour of seeming convalescence, surprises even the most wary with a sudden and overwhelming congestion of some vital organ?

"In this locality we also visited with Dr. Hays, Millaudon Potoon, black, male, aged fourteen years, who was said to have had a relapse, succeeding a fever of four days' duration. We saw him again the following morning. At neither visit did he have any fever. The skin was rather cool, temperature normal, pulse soft and very compressible, but not frequent. The appetite was feeble, strength much exhausted, mind spiritless and dejected. He answered questions willingly, but slowly and without animation. The decubitus was lateral, with the legs, semi-flexed, and we found him lying in exactly the same position, with the same air of utter indifference, on our second visit as we had left him on our first. He made no complaint, and on repeated inquiry admitted no special discomfort. His mother told us that he had been at work in the fields up to the date of his first attack; but the very great emaciation he exhibited was certainly not attributable to the brief acute attack he had experienced. He looked to me to be a well-advanced case of tuberculosis, and on inquiry I learned from Dr. Hays that his father had died of phthisis pulmonalis. Dr. Hays had never examined his chest, and his condition and the surroundings were such as not to invite my personal auscultation of him. A specimen of his urine exhibited, on the usual tests, an abundance of albumen."

In the letter, already twice quoted, Dr. Hays says: "In regard to the Patron boy (written Potoon in the published report), whom you saw on your visit here, his death took place on the twenty-eighth, just twenty-five days from the commencement of his fever, he having been taken sick on September 3. For ten days, or more, previous to his death, I am informed, he suffered from a profuse diarrhoea, which was never controlled. Being an emaciated, tuberculous specimen of humanity, he undoubtedly died from exhaustion."

Whether the guess at general tuberculosis, in which Dr. Davidson concurred, was correct or not in this case, is, in the absence of a more minute examination and record, purely conjectural; but will any one affirm that its necrology strengthens the suspicion of its being yellow fever?

Lastly—"At the Quarantine Station, which we reached at 9 p. m., we found, to our regret, the assistant quarantine physician, Dr. C. P. Wilkinson, down with the fever. He had been taken with the usual symptoms of chill, headache, pain in the back and legs, at 6 a. m. on Sunday, the twelfth, and when visited by us on Wednesday at 10 p. m. was, therefore, within eight hours of completing his fourth day. The quarantine physician, Dr. Finney, had kept an accurate record of his temperature—the *sole instance* in which we had the fortune to obtain it—from which it appeared that, on seizure, his temperature was  $100\frac{1}{2}$  deg. F. At noon the same day it was 103 and the same in the evening.

	Temp. F. deg.
Monday morning.....	101
Monday afternoon.....	104
Tuesday morning.....	101
Tuesday afternoon.....	104
Wednesday morning.....	101
Wednesday afternoon.....	104
And at 10 p. m., as taken by myself.....	103

He had, when we saw him, a hot skin; broad, moist tongue, covered with white fur; pulse 90, full, soft, regular; no precordial nor abdominal distress, nor vomiting, throughout the attack. He talked to us with unusual animation and energy that night, and the following morning when we visited him, a little after daybreak, we found him perfectly free from fever. His recovery was speedy and uninterrupted.

Dr. Wilkinson is a native of Louisiana, aged thirty years. He stated that he had an attack of yellow fever, in common with other members of his family, in 1855.

Is it possible to conceive a clearer or less disputable case of Febris Remittens Simplex than this? Yet Dr. Sternberg, without attempting to disprove, or even to deny, that it belongs to the same class as all the other fevers we saw—nay, if I understand English, indirectly affirming that it does—dismissed the *only case* in which the temperature record, on which he lays so much stress, had been kept with any fulness or accuracy in these words: “The temperature chart in the case of Dr. Wilkinson, as given by Dr. Finney, *certainly justifies* a diagnosis of remittent fever, but as already stated the history given me by Dr. Hays and Dr. Hebert of their cases, both upon my first and second visit, was of a continued fever.” I shall examine this affirmation presently. But, in concluding this analysis of all the disputable cases seen by the committee, I would ask whether I have not, in all fairness, made good the assertion of the majority report that “of the nature of the fever, without multiplying details, we would say that neither in its special features nor in their entirety could we realize a single prominent characteristic of yellow fever.” The cases of Haleoran and Gilmore showed the nearest approach to some of its phenomena, but the first died of epileptiform convulsions on the sixth day after complete defervescence, without the slightest febrile movement intervening; and in the latter a sudden and violent cerebral congestion supervened, and he died, we may say, with a bladder absolutely distended with urine containing not a trace of albumen.

There are a few pertinent considerations which I think strongly reinforce the position taken in the majority report, or at least leave the author of the minority report no ground to stand on—ground, be it noted, of his own choosing.

Dr. Sternberg, in the weakness of the particular proof, which he neglects to offer, rests his opinion largely upon the assertions that the fever was a *continued* fever—that a large percentage of the cases seen by him exhibited *luminous urine*, and that children who were least exposed to malarial influences constituted by far the greater number of patients. Let us examine these affirmations, *seriatim*.

In his first report, Dr. Sternberg says: “It is a continued fever of a

single paroxysm, lasting, it is said, from a few hours to four or five day. *No regular temperature observations have been made*, but from the statements of Dr. Hays, and from my own observations I am satisfied that the fever is of a mild grade and not characterized by remissions and intermissions." Leaving "Dr. Hays' statements" out of consideration for the moment, I think we may dismiss as valueless, for our present purpose, any man's judgment on the remittent or continued character of a fever, when that judgment is based upon one single and hurried observation alone.

In his second report, Dr. Sternberg, dropping the evidence of personal observation, repeats : "Temperature observations have only been made in a few cases ; but the history given me by Dr. Hays and Dr. Hebert of the cases which I have seen, is of a continued paroxysm, lasting from twenty-four hours to four or five days." And again, "as already stated, the history given me by Dr. Hays and Dr. Hebert of these cases, both upon my first and second visits, were of a continued fever."

Dr. Sternberg has failed to express himself with precision. For, of course, he does not mean to assert, what his language would seem to intimate, that either Dr. Hays or Dr. Hebert regarded a single case of theirs as of a continued type, or that they so declared. On the contrary, not only Dr. Hays and Dr. Hebert, but Doctors Jones, Westerfield and Ryan, in a word, every practitioner in the infected district, most positively and emphatically denied that they had any cases which they believed to be continued fever, and as unequivocally declared that all the cases they had seen were malarial fevers, such as they were in the habit of treating every summer. Dr. Hays, in several letters, has repeated this assertion, and in his latest communication, dated November 25, writes : "I have seen nothing lately to change my views concerning the fever of the lower coast, and close observation confirms me in the belief that it was purely malarial."

Dr. Sternberg evidently meant to say that the temperature observations which he secured from these physicians led him to conclude that the disease was a "continued fever of a single paroxysm." In his first report, however, he does not give us a single record on which to base this assertion, and in his second, only two—those of Gilmore and Haleeran. I have already reviewed these cases as presented in the majority report. Let us examine them by the

light of Dr. Sternberg's own transcript, and see if they bear out his conclusions :

" Gilmore, aged ten, was taken sick at *midnight*, Sunday night, September 12. No chill; temperature 106 deg. F.; Monday morning 105 deg., evening 104 $\frac{1}{2}$  deg.; Tuesday morning 104 deg.; Wednesday 3 p.m. 103 deg.; Thursday morning 101 3-5 deg."

Is there any expert living who would undertake to pronounce from such a record—which gives no account whatever of the very period at which remissions take place in the large majority of cases, viz., from sundown to sunrise—that this was certainly a *continued fever*? But Dr. Sternberg has not even transcribed the record correctly? He says that Gilmore was taken at midnight *without* a chill, and that his temperature was then 106 deg. I may have been in error in writing that he had "the usual light chill," though I took the account down on the spot, from his own mother. But where does Dr. Sternberg get his midnight temperature of 106 deg. F. from? There was no one in the house capable of taking it. Indeed, Dr. Hays had the only thermometer in the neighborhood, and he told us that he did not see the child "until Monday at noon."

And Halecan is placed in the same class. Now, according to Dr. Sternberg's record, he was "taken sick at 10 a. m., Sunday, September 12. Temperature Sunday noon 103 deg.; Monday morning 101 $\frac{3}{4}$  deg., evening 102 $\frac{1}{2}$  deg.; Wednesday morning 101 deg. F." It would be sheer trifling to discuss whether such a chart exhibited the curve of a continued fever.

But the point of greatest significance, in Dr. Sternberg's opinion, is the presence of albumen in the urine. On his first visit, he says that he found it in three cases. "The other cases in which no albumen was found were," he remarks, "too far advanced in convalescence or too early in the disease to make the absence of albumen a point of diagnostic importance." What the special differentia of these three cases were, we are not told. "In one of them," he says, "in which the most abundant deposit of albumen occurred, a boy of twelve—the boy was dressed and sitting up—had slight fever, glistening eyes, red spongy gums, and slight headache." Well! this boy with the most abundant deposit of albumen, and glistening eyes, and red and *spongy* gums, "*never went to bed at all*, and is now as well as usual," writes Dr. Hays in his published letter of October 22. No other information concerning them is given us in the report, which,

so far from offering any scientific data for judgment, is a mere collection of opinions and dogmata.

On his second visit he also found three cases of albuminous urine and we have discussed the reasons for doubting whether, in two of those cases, the leison could be fairly attributed to the fever. In the third, the specimen obtained was certainly untrustworthy. But, granting the albuminuria to have been a proper febrile leison in all of them is it conclusive as to the specific nature of the fever? Is albuminuria pathognomonic of yellow fever? Do we not find it in scarlatina and measles? Does it not occur in a number of pathological states, in cholera, diphtheria, erysipelas, in typhoid and typhus and puerperal fevers? As a transient phenomenon, is it not frequently seen in pregnancy, and as a result of acute alcoholism, of exposure to cold, and of a variety of unknown temporary causes? Is it true, as Dr. Sternberg insists, that it is a phenomenon only occasionally present in the high grades of malarial fever? Statistics on this point are, I confess, much needed. Those of us who practice in malarial districts must own to remissness in not having finally settled this question by carefully recorded observation. But data are not wanting to prove that in the congestive type of malarial fevers albumen is very frequently found in the urine. In the "algid" form of "pernicious fever," our *congestive fever*, Frerichs, who never saw a case of yellow fever in his life, I suppose, found it present in twenty out of fifty-one cases, 40 per cent nearly, and in five of these there was *suppression of urine*. After my return from this commission I examined the urine in three cases of remittent fever, in which the temperature rose to 105 deg. F. and upwards, and in one of them albumen was present. They were all of the congestive type.

The most remarkable statement, however, in Dr. Sternberg's reports is the following: "I would observe, however, that nearly all of the cases are young children, and that the adults, who by reason of their exposure in the rice fields would be most subject to malarial poisoning, have to a great extent escaped." And again: "The theory that this fever results from malarial emanations from the rice fields seems to me untenable from the history of the epidemic; from the fact that adults are most exposed to these emanations, while children are most subject to this fever," etc. Be it understood that the infected district is a low alluvial plain stretching away from both banks of the river, below whose level it lies, to the Gulf—with not an ob-

struction of hill or forest from marge to marge. The rice fields are coterminous with the enclosures of the cabins—and the one may be said to begin where the other ends. Whenever the prevailing winds are favorable, the miasm is wafted without let or hindrance through the house, and the child at the breast is as constantly and completely exposed to its influence, as thoroughly saturated with the fell poison as the horny-handed father in the field. The sole force that determines which shall first succumb is the capacity of resistance. But the children are more than normally in excess of the adults. The riparian belt of Plaquemines parish is the most prolific area in the State. Its fecundity has almost passed into a proverb. And it would not be unreasonable to expect that the larger class, other things being equal, would furnish the greater number of subjects. But it did not. How many children Dr. Sternberg saw with fever on his first visit, and what proportion they bore to the sick adults, he does not tell us. His reserve of facts on this point, as on all others, is singularly complete. But, on his second visit, I can affirm that of the whole number of sick examined by us during the two days, *only two were children* under fourteen years of age. Dr. Westerfield did inform us that the larger number of his cases were among children; but the direct opposite was stated by Drs. Hays and Hebert, when questioned specially on this subject. Dr. Hays, moreover, has removed any doubt that such general statements might still leave by giving in his letter to the President of the State Board of Health the exact proportion of sick adults and children seen by him up to that date. "From July 31 to October 22," he writes, "I find upon a careful examination of my visiting list, that I have had 123 colored and 95 white patients (a total of 218) with malarial fever. Of these fifty were white children and fifty-nine colored children fourteen years and under"—*exactly one-half* of the whole number treated. On the doctrine of probabilities it is not likely that Dr. Hays' experience is exceptional.

On this point, as on almost every other, whether of fact or opinion, Dr. Sternberg and the active practitioners of the district are at variance, with one apparent exception. His report concludes with the following paragraph :

"Dr. Wilkinson, Sr., the most experienced practitioner in the vicinity, who has been called to see many of the severe cases in consultation, made an unqualified diagnosis of yellow fever. He is perfectly

familiar with the malarial fevers of the country and has seen much of yellow fever, has had cases of malarial fever in his practice this fall, but considers the severe cases of continued fever which he has seen in the practice of Doctors Hebert and Hays as undoubted cases of yellow fever. Dr. Hebert evidently is much inclined to agree with Dr. Wilkinson. Dr. Hays insists that the disease is a malarial fever *of the same type as he saw in 1878*, which some practitioners in the vicinity called at the time yellow fever, but which he has never admitted to have been yellow fever."

It would be difficult to crowd more errors into the same number of lines.

Dr. Wilkinson can only be said to live "in the vicinity" by the loosest construction. His residence is at least seven miles above the point where we saw our first cases, and where Adrian Dragou died. The rice country begins below this settlement. Dr. Wilkinson lives in a sugar region. Now there is no growing crop that furnishes so complete protection against malarial emanations as cane. By June the plant has attained such size as to completely shelter the ground from the direct rays of the sun; it remains green during the entire summer and autumn; it requires thorough drainage, and is not taken off until cold weather. The plantations immediately above and below Dr. Wilkinson's are devoted to this culture on a large scale, and a little rice is planted only as an accessory crop. Hence the weakness of Dr. Sternberg's argument elsewhere in the same report, that the theory of the fever being due to malarial emanations from rice fields is untenable, because "in various localities where rice is cultivated, as in Dr. Wilkinson's practice, this fever has not prevailed."

Dr. Wilkinson was not called to see "*many* of the severe cases in consultation," and did not make "*an unqualified* diagnosis of yellow fever." Dr. Hays writes me, October 6: "Since he (Dr. Wilkinson) saw the Jourdan (Giordano) children—long before your visit—he has not seen a case of fever at Point Michel until September 26, when I called him in consultation for a boy (mulatto), who subsequently died of congestion of the brain. In speaking of the cases of fever at Point Michel he was particular *not* to call them yellow fever."

To the President of the State Board of Health, in his published

letter of October 22, Dr. Hays, writing to the same effect, affirms that—

"When Dr. Wilkinson visited the Giordano children, September 1, in consultation with Dr. Hebert and myself, he *then coincided with us in the opinion that it was malarial fever.* Thenceforward he saw no more of my patients until the twenty-sixth, when he visited, in consultation, a young colored man about eighteen years old, in the upper part of Point Michel. That patient died the morning of the twenty-eighth with congestion of the brain. He vomited some blood at 2 a. m. of the twenty-sixth, before Dr. Wilkinson saw him. He never vomited afterwards, had no suppression, no albumen. Dr. Wilkinson never told me that he thought that a case of yellow fever."

Dr. Wilkinson has not "seen much yellow fever." In the last twenty years he certainly has not seen an epidemic of it, if he ever saw one; and his experience has been limited to the occasional cases occurring below the city from time to time, more numerous in 1878 than for the two previous decades. Dr. Wilkinson may "consider the seven cases he has seen in the practice of Drs. Hays and Hebert as undoubted cases of yellow fever," but if Dr. Hebert be the truthful man I believe him to be, he is not "inclined to agree" *at all* "with Dr. Wilkinson." On the contrary, he told Dr. Davidson and myself that he had not seen a single case which he regarded as yellow fever. In a late communication from Dr. Hays, that gentleman writes: "Dr. Hebert told me, four days ago, that he had not pronounced any case of his yellow fever, and did not believe it to be such."

The charge that "Dr. Hays insists that the disease is a malarial fever of the same type as he saw in 1878, which some practitioners in the vicinity called at the time yellow fever, but which he has never admitted to have been yellow fever," is best answered by Dr. Hays himself. In his letter of October 22, referring to this assertion, he says:

"Two different times in his report Dr. Sternberg says that a similar fever prevailed in 1878, which some physicians called yellow fever, but Dr. Hays believes it also to have been malarial fever.

"Presumably he refers to Point Michel, and I am at a loss to know whence he derives his information, as I perfectly well remember that no other physician saw any patient of mine during the fall of 1878, either in consultation or otherwise, and consequently could

scarcely be expected to make a more accurate diagnosis than the physician actually in attendance on the sick.

"In 1878 I experienced no great difficulty in recognizing a case of yellow fever on the fourth of September, and subsequently three other cases, and, strange as it may appear to a scientific expert, did not confound the numerous cases of hemorrhagic malarial fever with those cases, although an epidemic was prevailing at the time in New Orleans."

As respects the etiology of the fever Dr. Sternberg is as characteristically confident as on all other points connected with it. He knows no reserve, has none of those hesitations or misgivings which perplex others in the extreme. Its source of origin was "the infected bark *Excelsior*," which anchored at quarantine for eleven days, from June 24 to July 5, and again from July 12 to August 16. The germ, instead of choosing the nearest point of disembarkation, the quarantine station shore, or setting out on its travels from the quarantine hospital, to which the sick of the *Excelsior* were taken, and where two of them died, crossed the river, about a mile wide, effected a lodgment on the batture, "from which about this time a disagreeable odor was observed to come," multiplied there, "the battures along the river bank, from (his) my point of view, furnishing favorable local conditions for the increase of the specific poison of the disease," and on the first of August, being ripe for mischief, gave Dr. Westerfield, whose area of practice lies opposite the quarantine station, a taste of its quality. On that day his first case occurred. Five days after, "six cases occurred in one family, one and a half miles below, then seven cases in another family about the middle of August," and at that date "Dr. Hays saw his first case, seven miles down the river from his house."

Now, I affirm, without fear of contradiction, that plausible as the germ theory is, and not devoid of value as a working hypothesis, there is not a scintilla of *proof* that the virus of yellow fever multiplies itself outside of the human body, any more than that of variola or scarlatina or morbilli. And to the theory of progression by aerial diffusion, or by personal contact, or house to house march, Dr. Hays' history of its outbreak in his area is absolutely fatal. In his letter of October 22, so frequently alluded to, he gives the following authoritative history of the outbreak in his section :

"The first case of the fever seen by me was Geo. Burton, native of Louisiana, white, male, aged thirty, a resident of Union settlement, on the left bank of the Mississippi river, about fifty miles below New Orleans. I was called in consultation by Dr. Hebert, July 31. The patient died with congestion of the brain August 2, having been sick about two weeks.

"The next cases were August 4, a colored woman and child, at Point Michel, right bank of river.

"August 8, Mrs. L., white, at Grand Prairie, left bank of river, fifty-five miles below New Orleans, and a white child at Point Michel, one and a half miles below the Lightell case.

"August 15, one case at Petite Prairie, right bank of river, fifty-eight miles below New Orleans.

"August 16, a case at upper limit of Point Michel, forty-seven miles below New Orleans.

"I give the foregoing details to show how the first cases made their appearance almost simultaneously on different sides of the river, at localities widely separated, and in families that to my certain knowledge held no communication with each other."

The first case in Dr. Hebert's practice, many miles above Dr. Westerfield's, occurred there two weeks before the first case of the latter. But there is no doubt that, although between the middle of July and the middle of August five cases had occurred in the practice of Dr. Hays, the endemic soonest gathered headway and made the most rapid progress below. The probable reason of this, as the majority report suggests, is to be found in the trend of the land and the nearness of the gulf, which determined a more rapid drainage and consequent exposure of decaying vegetable matters to the heat of the midsummer sun, after the water had been let off the rice, and is in strict consonance with all that we know of malarial conditions.

I shall not trespass upon your patience, already too much imposed upon, by repeating the generalizations of the majority report, pertinent as I believe them to be; but there are one or two points of the greatest diagnostic value, which cannot be omitted in summing up the evidence, which seem to me of prime value as aids towards arriving at that just verdict which a subject of such far-reaching importance demands.

The first of these is the remarkable fact that the feyer, through-

out its entire course, was absolutely *confined to natives and long residents*. Whether the type were mild or malignant, if the disease had been yellow fever the very opposite would have been probably the case. In malarial fevers, as is well known, the new comer enjoys the highest immunity. A certain point of saturation must be reached, it seems, before the poison manifests its peculiar phenomena in the human system. Now Dr. Hays' testimony on this subject is unimpeachable. In his published letter of October 22 he says emphatically: "At the beginning of the endemic this section was overrun by tramps of every nativity—Italians, Germans, Irish, Chinese and others—seeking employment in the rice harvest. They were here temporarily, and none of them were sick. Since the middle of September the coast has been lined with the luggers of the orange merchants, who have been busily engaged in gathering oranges in the yard and around the houses where lay the sick, and whose people have died of the fever. All their surroundings were favorable to the development of yellow fever if its poison existed in the locality, and the foreign boatmen, many of them unacclimated, were the very material for it to feed upon, and yet none of them fell ill."

Again, although I admit that it is extremely unscientific to make our diagnosis *depend* upon a greater or less percentage of mortality, it will not be denied that the *rate* of mortality, and, more especially, the *mode of death*, are important factors in making up our diagnosis in doubtful or questioned diseases. Dr. Sternberg dwells upon the mildness of the fever. Adopting the opinions of Beranger-Feraud, and desiring to force them upon others, he is compelled to do so, *ex hypothesis*. I admit, and the majority report stated, that the type, "for the most part" of those cases seen by us, was mild. The percentage reached in Dr. Hays' practice was one in sixteen nearly, or about 6 per cent. And this, be it understood, to quote the language of the majority report, in no wise exaggerated, "under bad hygienic conditions, the patients crowded in damp, dark, ill-ventilated rooms, seen usually late and necessarily infrequently, with no nursing or worse, lifted out of bed or seated up for every occasion, in every stage of the disease, and fed or starved, as fortune favored—improperly or unseasonably often—and as the waking or the whim of the nurse chanced."

But in the absence of *autopsies*, which would have revealed the

true pathology of the fever, we must fall back, as our only guide, on the *necrology*. Under these circumstances the manner of death possesses a significance which I think it would be difficult to over-rate.

From August 31 to November 25 Dr. Hays had 234 cases of the fever—100 white and 134 colored. Of this number 15 died—8 white and 7 colored. Yet of the whole not one had black vomit, or tarry stools, or suppression of urine; though, as he has testified, hematemesis, hemorrhage from the bowels and hematuria were not infrequent. With our knowledge of the pathology of yellow fever, of the rapid and profound degenerations it induces in the abdominal viscera—the spleen excepted—and the consequent obstruction to the gastro-intestinal and renal circulations, is it possible to conceive that out of fifteen deaths not one should have presented a symptom of these characteristic lesions. In their total absence among so many fatal cases is it not a legitimate deduction that the disease, whatever else it may have been, was *not* yellow fever?

Since this article was written, I have received a note from Dr. Hebert, dated December 3, in which he says: "I have treated about two hundred cases of the fever this summer, of which three died—two children besides Adrian Dragou. My opinion is yet unchanged, that the fever we have had this summer is *malarial* not *yellow* fever. I have not seen one case of black vomit, or suppression of urine, or yellowness of eyes or skin."

It may be argued that some of the cases were yellow fever, whilst others—the majority, perhaps—were malarial. The hypothesis seems to me utterly inadmissible. Nearly a century ago Rush recognized the dominant power of yellow fever to sway all other fevers to its own type. In his crisp epigrammatic phrase, "it drives out other diseases and forces them to put on its livery." The earlier cases—the Giordano children and Dragou—are specially claimed as yellow fever. Had they been, who is there familiar with the history of the disease that will not admit that in a very brief while the more virulent poison would have trampled all rivals under foot and ruled alone.

Of course, if we adopt the views of Beranger-Feraud, that what he calls "inflammatory fever, acclimating fever, benign yellow fever, abortive yellow fever, creole yellow fever, dengue," etc.—in a word,

all the climatic endemic fevers of a non-malignant type, are only a mild species of true yellow fever, "*very near*, if not identical with yellow fever"—whatever that may mean—the whole matter is so much simplified that he who runs may read. But its very simplicity creates distrust. No one contends that yellow fever presents always the same malignant aspect. Happily, in the very worst epidemics the majority of cases are not virulent; if they were, the disease would rival in its mortality the black death or the sweating sickness of the Middle Ages. But when he tells us of *epidemics* in which he lost three cases only in 400, and one case in 210, he puts a strain upon our faith in his nosology greater than it can bear. "The jaundiced eye sees everything yellow." Such epidemics may occur in "the Antilles and tropical America," but they are unknown in the history of the disease as it has prevailed in Mexico and the United States.

Malarial fevers have not been confined to the lower coast during the summer just past. They have prevailed with unusual intensity along the whole Southern seaboard, up the valley of the Mississippi, through all the lowlands of its tributaries, and in the city of New Orleans. Before the *Excelsior* had come up to our wharves I had seen a number of cases of severe bilious remittent and congestive fevers of the same type as those I encountered later at Point Michel. Before she had ever reached the mouth of the river I had attended, in consultation with Dr. D. W. Brickell, a case of congestive fever which died on the tenth day of the attack, after hematemesis and hemorrhage from the bowels, in a profound collapse, with the senses as acute and the intellect as clear as they were in health, *at the close of the third day of suppression of urine*. The remittent nature of the fever was conclusively established by the thermometric chart and the clinical phenomena. On the fourth and fifth days there was no fever at all. On the sixth day a virulent tonsillitis ushered in another paroxysm which after one complete remission terminated in fatal congestion of the abdominal organs. Neither of us had the least doubt of the nature of the fever, and the diagnosis was corroborated by the seizure of a younger brother of the deceased, a few days after, with a fever of identical type and equal violence at the onset, but which yielded to large doses of quinine after the third paroxysm; the recovery being prompt and complete. Both the patients had been subjects of malarial poisoning the summer before, and each had had

repeated attacks of fever in the winter and spring. Similar cases, I have reason to believe, occurred in the practice of others.

But why multiply proofs? I have no idea that they would convince the unbelieving if they were as the sauds for number and as strong as holy writ. Fear is the most unreasoning of all the passions, and there is no word to conjure with so potent as yellow fever. Give it to the winds and the whisper of one anile gossip would outweigh the oaths of all the faculty.

The National Board, without an hour's hesitation and with not a dissenting voice, accepted the dictum of a single man against the unanimous protest of all the practitioners in the infected district, and despite the judgment of nearly all the practitioners in the city of New Orleans. I do not wonder at it! There is a vast deal of human nature in man. This is a case in which the popular clamor is for execution, not judgment; and where the consequences involved are so tremendous deliberation will always be overborne by the urgent cry for action. Communities have no patience for musiag while the fire which may destroy them burns.

If such precipitation is shown by medical men, how can we expect dispassionate action from the laity? The president of the National Board, in his official report, states that the majority of the commission declared the fever to be of a "severe" type, whilst the fact is they declared it, and with instances, to be "mostly of a mild character." Of course we know him to be incapable of an intentional misstatement, but it shows how little critical was the attention bestowed on the report itself. The vice president of the association, in a popular article, did not exaggerate the truth when he declared that "no amount of assurance given by citizens, physicians or sanitarians" of New Orleans, would give confidence to interior towns. But he was very seriously in error when he gave as a reason "the policy of concealment which has been pursued so long and so uniformly." The present Board of Health can defend itself; but it is a matter of notoriety in this community that such a policy was certainly not pursued by the late Dr. Choppin. From the hour he took that high office, in 1877, he administered the duties of the Board of Health with the most rigid and uncompromising honesty. He carried out the letter of the law under which he acted to the least iota. He had no disguises; he ad-

mitted of no concealments. He swept out of the way any officer, no matter what his political or social backing, who proved obstructive by incompetence or infidelity. A braver, manlier, more truthful spirit never trod this earth. To all entreaty and remonstrance he was obdurate as a wall, and the man did not breathe who would have dared approach him with a bribe. He sacrificed to the discharge of his duties the most lucrative professional income, and the largest professional popularity. He brought down on himself the unappeasable wrath of the press, and day after day, and month after month, was threatened with assassination by those who fancied themselves injured by his stern fidelity to the law. Against all the storm he steered right onward, and sacrificing to his sense of duty the health and strength which promised so useful a future, he at last laid down his life with his office. When such charges are brought hereafter, let his honored name at least be excepted.

I have touched upon this point as strictly germane to the proper question before us. For we will certainly have the same hemorrhagic fevers here year after year, whenever the meteorological conditions are favorable, and if the judgment of those most familiar with the endemic diseases of Louisiana is to be set aside as unworthy of confidence or credence, the commercial future of New Orleans can be easily predicted.

Finally, we have had no epidemic of yellow fever in this city during the past summer, unless the medical profession of New Orleans is grossly incompetent to distinguish between yellow and paludal fevers, or has engaged in a general conspiracy to deceive the public; a charge which, I take for granted, no self-respecting person would bring against so numerous and reputable a body of gentlemen. But if the fever of the lower coast was yellow fever, and the National Board of Health has decided that *it was*; if the poison of the Excelsior was so nimble and so full of subtle flame that in spite of every precaution it infected the whole area from Pilot Town to Point Michel, then the National Board of Health is bound to show, on peril of its existence, why New Orleans enjoyed such immunity. How, without isolation, disinfection, or detention of persons and things, with uninterrupted intercourse by land and by water, by day and by night, between the city and the infected district, lying at our very gates, we escaped. And why the sick brought up from the coast—and I attended such myself—and lying ill with this fever, and some of them dying in the

very heart of the community, did not infect their unacclimated attendants and neighbors. Failing in this, the National Board will have committed *hari-kari*, for it cannot evade the alternative that the system of quarantine, in theory and practice, is a useless farce, imposing at great cost to personal freedom and commercial prosperity, a grievous and needless burden on the community ; a wretched relic of barbarism that should be swept away, as with the besom of popular indignation, into the limbo of forgotten rubbish.





